

Southwold, and Bramerton) yields a Rhizopodal fauna somewhat similar to that of the Red Crag.

The few kinds of Foraminifera yielded by the Chillesford Crag, a deposit regarded by Messrs. Wood and Prestwich (Quart. Journ. Geol. Soc. vol. v. p. 350) as probably contemporaneous with the Crag of Norwich (Uppermost or Mammaliferous Crag), indicate a rather shallow and cold sea (perhaps somewhat brackish too) as their probable habitat. They are *Polymorphina lactea*, *Bulimina elegans*, *Truncatulina lobatula*, *Rotalia Beccarii*, *Polystomella crispa*, and *P. striatopunctata*. Mr. Prestwich's observations (*loc. cit.* p. 351) on the probable influence of cold currents from the northern seas on the fossil fauna at Chillesford coincide with the above remarks.

Lastly, some Foraminifera collected by H. C. Sorby, Esq., F.R.S., from the Bridlington Crag*, some years ago, and kindly lent to us, have to be noticed. These comprise *Cornuspira*, *Miliola*, *Lagena*, *Dentalina*, *Cristellaria*, *Polymorphina*, *Cassidulina*, *Truncatulina*, *Polystomella*, and *Nonionina*, and are the most conspicuous of a probably more extensive fauna, nearly allied to that of the Suffolk Crag.

X.—*On the Law of the Production of the Sexes in Plants, Animals, and Man.* By Prof. THURY, of Geneva †.

M. THURY's memoir is divided into three parts. In the first, entitled "Deduction of the Law of the Sexes," the author indicates the course of ideas which has led him to his theory. The second, which is shorter, contains, under the title of "Résumé," the complete exposition of the author's notions. The third is a "Notice," prepared by M. Cornaz, in which this clever agriculturist describes the experiments which he has made, during two consecutive years, for the verification of the author's theory, and by which the latter has been completely confirmed.

The limits of this article do not allow of our following the author through the whole series of reasonings by which he establishes his theory. We shall only state that the study of plants, in which, by the management of the influence of external agents, the observer is enabled to instigate the development of either one or the other sex, seems to prove that the develop-

* Mr. Bean wrote of the Bridlington Crag in 1835 (Mag. Nat. Hist. vol. viii. p. 355), and Sir C. Lyell in 1839 (Mag. Nat. Hist. new series, vol. iii. p. 313. See also Phillips's 'Geol. Yorkshire,' 1829, vol. i. p. 69; and H. C. Sorby's paper on this Crag, in the 'Proceed. West Riding, Yorkshire, Geol. and Polytech. Soc.' 1857, iii. p. 555.

† Translated by W. S. Dallas, F.L.S., from the abstract by Prof. Pictet in the 'Bibliothèque Universelle,' September 20, 1863, p. 91.

ment of the male sex is always related to those general causes which induce a more complete maturation of the juices and a more perfect development of the organs.

This fundamental fact the author applies to the animal kingdom. He refers, in the first place, to the fundamental identity of the two sexes—an identity which allows us to explain the characteristic differences of the sexes by simple differences in the mode and amount of development. He then seeks the causes of these differences, by analogy with plants, in the conditions which, at a certain moment (very near the first origin of the organism, since it is anterior to the determination of the sex), produce a more complete development in the case of a male, and a less advanced or less complete development in that of a female.

It remained to fix the precise moment at which this primary determination of the sex takes place. This might be before fecundation, or during or after this act. In the former case, if the fecundation were retarded, this retardation, permitting a more complete development of the ovum, must generally induce the production of male individuals. Now, in bees, according to the observations of Huber, if the fecundation take place early, workers (*i. e.* females) are chiefly produced; whilst, if the fecundation be retarded beyond the twenty-second day, all the eggs deposited are male eggs. According to M. Thury, the decisive moment for the production of the sex will therefore precede the act of fecundation.

It is true that, in bees, the interpretation of the facts is very complex, partly on account of parthenogenesis, partly in consequence of some other peculiar circumstances in the reproduction of these insects. But the author also knew, from some previous experiments, that, in domestic poultry, the eggs last laid nearly always furnish the cocks of the clutch; and he thought it probable that the last eggs which detach themselves from the ovary of the fowl are those which have had the most time for maturation. These eggs are fecundated, as all physiologists are aware, during their passage through the upper part of the oviduct. Therefore here also, when the fecundation is retarded, males are the result.

It was easy to apply the preceding data to the uniparous Mammalia. In these the ovum separates from the ovary at the commencement of the rutting-season, and it may be fecundated at any time during the whole period that the female continues in heat, and consequently when its maturation or development is more or less advanced. If the fecundation take place at the commencement of the period of heat, a female is the result; if at the end of this period, a male. This is the conclusion which is fully justified by the experiments of M. Cornaz.

It is plain that, according to the author, the life of the un-fecundated ovum is divisible into two periods. In the first of these it is in principle a female ovum, in the second a male ovum. The turning moment (*moment de vire*), according to the author, is the time (probably very short) which separates the two periods, and in which the natural course of development induces some sudden change, the nature of which histology should reveal to us. He assumes that the relative duration of the two periods of the life of the ovum may be modified under the influence of the organic state of the female, whence would result a predisposition on the part of some females to give birth either to individuals of their own sex or to males. Temperature, by its direct action upon the ovum, and the influence of the fecundating male upon the organic condition of the female, would also produce similar results.

The author, in all his deductions, appears to start from a general point of view, which he certainly indicates, but nowhere demonstrates in a positive manner, regarding it apparently as a sort of axiom. He assumes that "*sexual life, being common to animals and plants, must be subjected to identical fundamental laws in both kingdoms.*" If this be true of the two kingdoms, it must apply with still more reason to the various divisions of the same kingdom. This admits of much generalization; but (and this is the difficulty) we have yet to distinguish with certainty the facts which bear upon essential laws from the infinitely varied manifestations by which these same laws are realized in combination.

The second and third parts of M. Thury's memoir are here reproduced entire.

Second Part. *Summary and Practical Observations.*

1. Sex depends on the degree of maturation of the ovum at the moment of its fecundation.

2. The ovum which has not attained a certain degree of maturation, if it be fecundated, produces a female; when this degree of maturation is passed, the ovum, if fecundated, produces a male.

3. When, at the rutting-season, a single ovum separates from the ovary to descend slowly through the genital canal (as in uniparous animals), it is sufficient that the fecundation takes place at the commencement of the rutting-season to produce females, and at the end to produce males—the turning-point of the ovum occurring normally during its passage in the genital canal.

4. When several ova separate successively from the ovary during a single generative period (multiparous animals and oviparous animals in general), the first ova are generally the least developed, and produce females; the last are more mature, and

furnish males. But if it happens that a second generative period succeeds the first one, or if the external or organic conditions change considerably, the last ova may not attain to the superior degree of maturation, and may again furnish females.

Ceteris paribus, the application of the principle of sexuality is less easy in the case of multiparous animals.

5. In the application of the above principles to the larger Mammalia, it is necessary that the experimenter should first of all observe the course of the phenomena of heat in the very individual upon which he proposes to act, in order that he may know exactly the duration and the signs of the rutting-season, which frequently vary in different individuals.

6. It is evident that no certain result can be expected when the signs of heat are vague or equivocal. This is scarcely ever the case in animals living in a state of freedom; but cattle in the fattening-sheds or in the stable sometimes present this abnormal peculiarity. Such animals must be excluded from experimentation.

7. From the mode in which the law ruling the production of the sexes has been deduced, it results that this law must be general and apply to all organized beings,—that is to say, to plants, animals, and man.

It is necessary to distinguish carefully the law itself (1 and 2 of this summary), which is absolute, from the applications of it which may be made with more or less facility.

Third Part. Notice by M. George Cornaz.

I, the undersigned, George Cornaz, administrator of the estate of my father, the late M. A. Cornaz, President of the Agricultural Society of "La Suisse Romande," at Montet, in the Canton de Vaud, certify that I received from M. Thury, Professor in the Academy of Geneva, under date of the 18th February 1861, some confidential instructions the object of which was an experimental verification of the law which governs the production of sex in animals.

I have applied to the management of my herd of cows the data furnished to me by M. Thury, and obtained *at once, without any uncertainty, all the expected results.*

In the first place, in *twenty-two* successive cases, I wished to obtain heifers; my cows were of the Schwitz breed, and my bull a pure Durham; the heifers were in demand amongst breeders, and the bulls were only sold to the butchers. I obtained the desired result in *all* cases.

Having subsequently purchased a cow of pure Durham breed, I desired to obtain from them a new bull, which might replace the one which I had bought at great cost, without waiting for

the chance of the birth of a male. I operated in accordance with the directions of Prof. Thury, and the success again confirmed the truth of the process which had been communicated to me—a process the application of which is direct and very easy.

Besides my Durham bull, I obtained six other bulls, of a cross-breed between the Durham and Schwitz, which I intended for work: by selecting cows of the same colour and size, I obtained very well-matched pairs of bulls.

My herd consists of forty cows of all ages.

To sum up, I have made in all twenty-nine experiments according to the new process, and all have given the desired product, male or female: I have had no case of non-success. All the experiments were made by myself, without the intervention of any other person.

I can consequently declare that I regard the method of Prof. Thury as real and perfectly certain, hoping that he will soon be able to profit all breeders and agriculturists in general by a discovery which will regenerate the business of cattle-breeding.

(Signed) G. CORNAZ.

Montet, Feb. 10, 1863.

XI.—*On the Process of Mineral Deposit in the Rhizopods and Sponges, as affording a Distinctive Character.* By G. C. WALLICH, M.D., F.L.S., &c.

IN a paper published in the Number of the 'Annals' for December last, Professor Max Schultze adduces evidence in support of the opinion that the siliceous spicules found within the chambers of certain Foraminiferous shells do not constitute integral portions of these organisms, but are the products of entozootic sponge-growth,—the evidence in question being based on the strictly Foraminiferous type of the shells in which such spicules occur, on the presence of the latter being only occasional, on their position and distribution when met with, and on the characters of sponge-sarcode as compared with "the organic substance remaining after specimens [of *Polytrema*] preserved in spirits" have been decalcified by subjection to dilute hydrochloric acid.

But whilst this may be regarded as the circumstantial evidence in the case, the opinion advocated by Professor Schultze appears to me to be sustained by proofs of a more direct and generally applicable nature. These I shall now proceed to notice.

According to Dr. Bowerbank*, "in the early stage of their

* "On the Anatomy and Physiology of the Spongiadae," 'Philosophical Transactions of the Royal Society' for 1858, p. 281 *et seq.*